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Interactive comment on "Drivers of Pine Island Glacier retreat from 1996 to 2016" *by* Jan De Rydt et al.

Anonymous Referee #3

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Review of "Drivers of Pine Island Glacier retreat from 1996 to 2016" by J. De Rydt et al.

The manuscript "Drivers of Pine Island Glacier retreat from 1996 to 2016" by J. De Rydt and colleagues analyses the role of several processes in causing the observed changes of Pine Island Glacier between 1996 and 2016 using numerical modeling. They estimate the relative role of calving, ice shelf thinning, ice shelf and ice sheet thinning, and well as combinations of these changes and their ability to reproduce the observed changes in ice velocity. The manuscript is well written, well explained, the figures are appropriate and this work is important as we try to better understand the glacier's changes in this region. There is one major point, however, that I would like to see addressed to make sure the results are robust and the conclusions not impacted

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by model parameter.

The 1996 velocities are reproduced by inverting simultaneously the rigidity parameter A and the basal friction parameter C. This has been done in several studies, however there is an infinite combination of these two parameters that can yield similar velocity fields with drastically different values for each of the parameters. This is something that is observed for the inversion of 2016 in the present manuscript: two additional inversions are done to fit the velocity observations for that year, one changing only C, and the other one changing only A. Therefore, two different combinations of parameters (A1996 with C2016 and A2016 with C1996) yield similar velocity fields in agreement with the 2016 observations. This is a recurrent problem in our field and the alternatives are limited, however I am wondering if a different combination of parameters would lead to different conclusions and I would like to see this point addressed. One solution to do so could be for the 1996 observations to first optimize the friction parameter and then the rigidity parameter in a first case, while a second case would first optimize the rigidity parameter and the friction afterwards. I expect these two cases to exhibit significant differences as most of the misfit will be captured by the first model parameter inferred, with the second one only capturing "residual-like" misfit. Experiments done with these two initial conditions will allow to make sure that the conclusions are robust and not impacted by the choice of these parameters.

Major comments

As explained in detail above, I would like to make sure that the conclusions are robust under a range of values for A and C as both parameters are unconstrained. Additional experiments with other values such as proposed above would help ensure that this is the case.

The role of grounding line retreat is confusing. It is not part of ice shelf thinning, but it is included in the ice sheet plus ice shelf thinning. Would it be possible to separate it more from the other processes? The grounding line retreated significantly between

1996 and 2016 for this glacier and it would therefore be interesting to know how much the grounding line evolution contributed to the acceleration. The text about grounding line is also not always clear: it is not included as a separate process, but it is sometimes mentioned along other processes (e.g not mentioned in the abstract, previous studied about grounding line retreat only mentioned in the introduction, stated separately from the rest in conclusions, etc), so I would like to see addressed in a similar way throughout the manuscript. Part of this confusion might come from the problem that grounding line retreat is not clearly separated from other processes.

These experiments investigate the instantaneous response to changes in geometry. In some places of the text, it is clearly stated, but in other places it is not clearly stated. I would also like to see more discussion on this aspect to assess how this could impact the results, especially how the limitations in the model and observations could impact the results.

Minor comments

p.3 l.65: How linear do you expect the different processes to be? Is that something that can be investigated a bit more? Also, with what processes are the impact of grounding line changes included? Same for l.71-72.

p.3 l.74: Despite a lot of limitations, calving has been included in some models for a long time now (Martin et al., 2011) and a growing number of ice flow models are starting to include it at large scale in various forms (Seroussi et al., 2019).

p.3 l.75: I was wondering if this should not be: Delta Ua= Delta Uc=0

p.3 l.86: What knowledge is referred to here? As mentioned on l.87, it cannot be estimated from observations, I am wondering where this comes from?

p.5 Fig.1c: How do you explain the thinning just downstream of the 1996 grounding line?

p.6 l.151: U* is a little confusing, it might be more clear to list the perturbation experi-

ments or explain a bit more what the asterisk refers to.

p.7 l.167: I would like to see more information on the inversion for A and C. What are the initial values, what are the minimum and maximum values admissible, . . . ?

p.7 l.173: What about the thermal regime? What is assumed for that? Many models of Pine Island have been developed over the past few years, so I guess it would not be difficult to use the ice thermal regime from a previous model to get a first guess of the rheology?

p.9 l.237: It is not clear which “earlier conclusion” is referred to here.

p.10 Figure 3e: How gate-dependent are the results? What would happen if gates a few kilometers upstream or downstream where used?

p.11 l.265: Is that what is expected in that case? Because the effect accumulates in time over 20 years and, even if it is reflected in the 2016 geometry, limitations in model parameters and errors in observations might limit the ability of the model to simulate good instantaneous response.

p.13 l.308: As mentioned above, you have here two sets of parameters (A2016 with C1996 and A1996 and C2016) that reproduce the 2016 velocity field. There is an infinite number of combinations that can reproduce a given field, so given the limited constraints provided for the inversion of A and C, can we make sure that the results are not dependent on the combination of parameters used?

p.17 l.397: The limitation of Weertman sliding is mentioned several times, but not really discussed, so it would be nice to see a bit more discussion on that.

p.17 l.403: It should be specified that this is an instantaneous response.

p.17 l.407: I thought that the results attributing the remaining changes to rigidity or friction were inconclusive as the fields computed were unphysical?

p.18 l.436: So what is used in this region and for the transition?

p.19 453: Why use a uniform value of -15 degrees?

p.19 l.463: What are the constraints used for A and C (minimum and maximum values possible)?

p.22 l.505: How about the relative weight for the two parts of the cost function referring to A and C? How are the weights calibrated?

Technical comments

p.4 l.121: we required -> we needed

p.4 l.122: How about grounding line positions?

p.6 l.160: It would be good to specify where you infer A because different groups use different parts of the domain (entire domain vs floating parts only)

p.8 l.198: missing Delta in front of UCalv or Delta UThinCalv)

p.11 l.262: it would be more informative to provide numbers about the different between Delta UThinCalv and Delta UCalv + Delta UThin rather than just saying they are about the same.

p.11 l.275 and l.277: hypotheses -> hypothesis

p.14 Figure 5d: Would it be possible to add results for more exponents? At least the value for m=21 to see the highest change.

p.17 l.406: increases -> increase

p.17 l.413: the DOI is missing for now.

p.18 l.338: the finite element method

p.18 l.438 to l.445: references are missing for the stress balance approximation, inverse capabilities, . . .

p.20 Figure B1c: It would be good to see the temperature equivalent to the rate factor

on top of the rate factor on the colorbar.

On all the spatial figures, the x and y axis are psx and psy, which looks a bit awkward to me, but that's not very important.

References

Martin et al., The Potsdam Parallel Ice Sheet Model (PISM-PIK) - Part 2: Dynamic equilibrium simulation of the Antarctic ice sheet, doi: 10.5194/tc-5-727-2011, The Cryosphere, 2011.

Seroussi et al., initMIP-Antarctica: an ice sheet model initialization experiment of ISMIP6, doi:10.5194/tc-13-1441-2019, The Cryosphere, 2019.

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